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Title	The effect of the Fort McMurray wildfires on the health of evacuated workers: follow-up of two cohorts
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Reviewer 1	Dr. L Reifels
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General comments (author response in bold)	Thank you for the opportunity to review this interesting manuscript. This manuscript could make a potentially relevant contribution to the wider literature, but my main concern is that is relatively difficult to follow and more efforts could potentially be made to guide the reader through the labyrinth of different measures and time points. In addition to the relatively small sample size, I am not certain that overarching cohorts have been sufficiently clearly distinguished from study samples (or participants), which may benefit from further clarification (as it is not full cohorts that are being examined, but samples derived from these). **The text has been rewritten to make this distinction very much clearer** In order to consider the manuscript for publication, my main comments and suggestions to further improve the manuscript are therefore as follows: Abstract: 1. Background section: could be more specific, e.g., '…to rapidly assess the health effects and experiences…' 2. Method section: should indicate the general types of health outcomes and experiences examined (and perhaps also briefly hint at measures used and types of data analysis conducted) **We accept these comments. We have edited the Abstract to take into account as many comments as we can but the program to upload the abstract will not accept >250 words and we are not able to do more within this word limit.** Introduction 3. The introductory paragraph would benefit from a literature reference on the broader (health and/or other) impacts of the Fort McMurray fires **We have revised the Introduction to take account of this comment and one from Reviewer 2. There is nothing yet published on the impacts of the fire.** 4. For an international audience, the inclusion of an illustrative map of fire-affected areas (and possibly main migration routes) may be worth a consideration **We have addressed this possibility in our letter to the Editor.** Method 5. Please provide a brief rationale for combining both cohort samples **We have done this in the paragraph describing study participants. Inclusion of the trade cohort helps to more truly represent the transient workers characteristic of the area, which we feel to be important.** 6. Language use throughout main text and tables should more clearly distinguish overarching 'cohorts' from your respective 'cohort samples' (or participants) **This has been addressed by restructuring the paper to concentrate on the Post-Fire Study Group (P-FSG) rather than the cohorts of origin of the study group members.** 7. Please provide further information on the response rate among eligible (in-scope) cohort members, and the respective representativeness of your samples of these broader cohorts (alternatively, the lack of relevant information on these aspects should be discussed as a study limitation) **We have addressed this by adding the response rate (75%) for the P-FSC members drawn from the injury cohort and raised the lack of a true denominator for the injury cohort for whom we had only the permanent address not the temporary address (often a work camp) and so do not know who, other than respondents, were in the area at the time of the fire. Paragraph 4 of the Discussion/Interpretation addresses the likely unrepresentativeness of the sample.** 8. Data collection: the comparatively broad timeframes (and at an individual level potentially highly variable timepoints) for post-fire data collection (22 May-30 October and 4-24 weeks) may be an issue that could briefly be discussed (since timing of post-disaster data collection does matter and can impact on outcomes) **This is discussed in relation to mental health in paragraph 2 of the Discussion/Interpretation.** Results 9. In text references to Table(s) 1A, 1B and 1C (instead of Table 1) are slightly confusing, as these imply the existence of separate tables (and Table 1 does not contain designated sections that are labelled as such) **Agreed. This is now table 2 but labelled appropriately** 10. Table 1 heading: suggest changing heading to 'Demographic characteristics of study participants (both cohort samples combined)' or similar – as the current table heading and layout (and corresponding 2nd paragraph of the Results section) could be misinterpreted as referring to a comparison of cohort demographics **We have made this change (now table 2) to refer to the Post-Fire Study Group rather than the cohorts of origin.** 11. Tables 3 & 4: for tables to be standalone, clearly specify that anxiety and depression scores are based on HADS (and provide full acronym wording, e.g., as table footnote) **Changes made (tables now 4 and 5)** 12. Table 3B heading: if these are follow-up data, the heading should state '…at follow-up' **Change made. Now table 4B**

	13. Table 4: table heading (and corresponding text section on p.7) should specify the time point for HADS scores (e.g., at evacuation, or follow-up) **Change made (HADS only collected at follow-up). Now table 5.** 14. It is not sufficiently clear to me if you have access to HADS data from more than one time point (e.g., pre/post-event data) which could enable relevant pre-post comparisons **Now specified in Methods that information available only at follow-up** 15. Table 5 heading: please specify that data relate to 'those in the injury cohort sample evacuated from Fort McMurray' (similar to Table 4) and the time point **Changes made (now Table 6).** 16. Anxiety and Depression (p.8, last paragraph & Table 5A/B): Table 5A suggests that 'time since fire' (p=0.09) was not significantly related to anxiety (but only in stepwise regression under 5B). Similarly, depression and financial loss only appeared to be significantly related to depression in step-wise analysis (5B), but not in 5A. This could be further clarified in text. **Further clarification inserted.** Interpretation 17. (p.8, first paragraph): the sentence starting with 'Anxiety was greater in those completing…) may benefit from rewording to improve clarity **This has been reworded in an attempt to clarify** 18. Study findings could be discussed in the context of any other existing research on the (health) effects of disaster evacuations (if this literature is indeed scarce, it may warrant a future research recommendation, and strengthen the contribution of the current paper) **There are papers on the effect of physical barriers to escape (for example from the Twin Towers) and I have added a reference but not, that I have found, on the effects of the type of accommodation.** Minor typographical errors 19. Abstract (Interpretation, last sentence): 'healthy worker' **Corrected** 20. (p.9, last paragraph): 'they were generally' **Corrected** 21. Reference 6: check spelling of 'Kaniasty' **Corrected** 22. Table 1 (Demographic factors): 'N=130' **Corrected (now Table 2)**
Reviewer 2	Mr. Adam Vaughan
Institution	Simon Fraser University, School of Criminology, Vancouver, BC
General comments (author response in bold)	Literature Review (major comments) 1. Though I appreciate the fact that only two fatalities were related to the fire, the impact was much broader. The authors touch upon this impact on residents but do not explicitly state why the study is important. For those not familiar with the devastation of the fire, in addition to the 88,000 evacuees, the authors may want to include some additional temporal, spatial context surrounding the fire as well as perhaps some sort of measure of the fires impact (e.g., # of buildings damaged etc.). This information may be suitable as an Appendix but would be useful to readers who are not familiar with the make-up of Fort MacMurray. **The Introduction has been revised to add more about the impact. A suggestion has been map to the editors to include maps showing the special context** 2. The only other citation that I could find in the literature review/introduction was the Fergusson et al., paper. Though they may not have used similar methodologies, there have been other Canadian studies that look at the psychosocial impact of being involved in a natural disaster that may be useful at framing why this particular paper is important. Susan King's work did not look at workers per se, but she and her colleagues did follow the longitudinal impact of the Quebec Ice Storm in 1998. **We have added two new references, to Suzanne King and to a group from Lethbridge who have looked at the effect of wild fires on community resilience.** 3. Do the authors have a reference that highlights that the impact of major disasters on transient populations is different from more settled groups? This makes sense, but the authors do not mention what proportion of the Fort MacMurray population is transient/shadow population of the 88,000 that were evacuated. **The new reference 1 estimates the number of transient workers and I have used this to make this important addition.** 4. The last sentence of the introduction was a little confusing. Were the authors interested in mental or physical health and not cases where both occurred? **Wording amended** Methodology (major comments) 5. My main comment/area of concern for the paper was this section. I struggled with understanding the timelines for data collection for each group and when this occurred. I referred back to Appendix 1 (which was quite helpful) multiple times to try to connect what was being written into the methods section. It appears that both the Injury and Trade cohorts. Were these two groups part of another study? If so, would the authors be able to cite these papers? **We have clarified this by adding a new table (Table 1) and by rewriting the methods section to concentrate on the Post-Fire Study Group (P-FSG) rather than the source cohorts.** 6. A small figure/table of timelines of when the fire hit, when folks were evacuated and when the survey instruments were given over time may aide the reader in following the research procedure listed between lines 33-54 on page 4 of the manuscript. **Evacuation was on May 3rd (Sentence 1 of the Introduction). The new Table 1 makes clearer the**

time between the start of the fire and data collection.

7. The last line in the second paragraph on page 4 (Line 47) appears to be in need of some editing.
This section has been rewritten and the new Table 1 added to clarify this issue.

8. The last sentence on page 4 where the authors state that the HADS was not included for the trade cohort became a sticking point for me as on page 5, in the statistical analysis section in the second last sentence (line 54) the authors state that the anxiety and depression scores were retained and entered into multivariate regression analysis. How was this possible if the data were only collected for one of the cohorts? Were ANOVA's run on the whole sample? Each cohort? This final section of the methodology needs to be made clearer.
We have now specified more clearly in the Statistical analysis section (and throughout) that the HADS analyses were only carried out for study group members originating from the injury cohort.

Results (minor comments)
9. For the most part, the results section was well done. Were there any cross tabs completed with associated tests of significance that the authors could highlight in Table 1? This information would be helpful for the reader to know particularly when moving on to the regression tests that are presented on page 8.
There were cross-tabulations done as a first step in the analysis but it would be difficult to include them in Table 1 (now table 2). As is evident from comparing (new) table 5 with (new) table 6A there was correlation among the independent variables, with only 2 of the 6 identified as related to anxiety score in a bivariate analysis being retained in the multivariate model. As men were more likely to complete later, this confounded the relation between anxiety sand time of completion and adjustment for sex allowed the relationship to be seen more clearly. We could not think of a way to usefully extend the descriptive analysis within the space constraints of the journal.

10. In the events during the fire section, how was it that the North direction which included 30 participants only involved 27.5% of the sample whereas South evacuees (n = 7) made up 67%? Was there any missing data in this specific section of Table 1?
I regret this was an error in transcription, now corrected from 7 to 73.

11. I found the results on page 8/Table 5A to be quite interesting. More specifically, line 36-44 has important health policy implications for future responses to similar disaster situations not only in Canada but around the world.
Thank you – we agree.

Interpretation (minor comments)
12. This was by far the best section in the paper. The authors raise a valuable point on page 9, line 26 re: pre-existing conditions. Did the authors control or have access to information that would have identified the severity of other pre-existing psychological conditions in the cohorts?
The only data we had for all the study group was on type of medication pre-fire and the reason for which it was taken. Only 4 were taking medication for mental health issues pre-fire.
I ask this as on page 10, lines 19-23 the authors refer to resilience and its relationship to employment status and physical well-being. Are the authors stating that these two factors protected/insulated/etc. these workers from other groups that were involved in the fire? I don't believe there was comparison data with other civilians.
The rationale for including this (apart from being, I think, likely) is that we know that high PTSD rates are being reported (but not yet published) in self-selected evacuated groups. Our statement is to allow for the possibility that the two groups differ in resilience. We have ourselves no comparison group of non-workers to test this.
Neira's work listed below would suggest that all groups are susceptible to trauma following disaster (human made or naturally occurring).

Agreed – but he also does discuss how some groups (the poor, first responders, children) tend to be at increased risk of PTSD after major disasters.

13. I was pleased to see Yuval Neria's work cited. He has a more recent paper on the longitudinal impact of the terrorist attacks on 9/11 in a 10-year follow-up study on the longitudinal impact of PTSD. He and his co-authors looked specifically at the impact of PTSD on workers who responded to the event (clean-up crews etc.) as well as referencing other mental disorders that can emerge from these types of events.
We are grateful to the reviewer for calling attention to this reference has been added. We were cautious about drawing too many parallels with 9/11.

14. Near the end of the manuscript, it may be useful to have a sentence or two re: the implications of this research. For example, how can the findings from this work help us – either the participants moving forward or others in the future who may undergo a traumatic event? It may sound cliché, but future research in this area seems to be a natural next step, particularly with the Injury cohort as this makes up the bulk of your sample.
The members of the trade cohort are being followed up as indicated by the protocol for that study and will be completing a final questionnaire (including the HADS) at the end of this year. It would be feasible to re-contact those drawn from the injury cohort although we need to balance the interest in doing so against the feeling that they should be allowed to move on with their lives. The message we want to give - to those who might undergo something similar and to physicians and employers who may be in contact with disaster 'survivors' - is that involvement in a disaster is not necessarily going to lead to long term difficulties.